advance; for example, in the valley of the Donau. Especially in regard to the Caucasus, his investigations in the region convinced him that no people already sufficiently civilized to employ metals could have passed over this range; and, on account of the geographical relations, we must assume that the Aryan peoples first divided in central Asia, and separated widely along the northern coast of the Aral and Caspian seas, and then proceeded through modern Russia, where the characteristic bronzes are not found, or westward through Asia Minor. Once in Greece, it is highly probable that Italy was their next step. A fact brought forward by Hochstetter in support of his theory — viz., the lack of ribbed bronzes, Mestea dicordoni — has proved a mistake. A point of attack is presented by the same investigator, in his assertion that the discoveries at Hallstadt do not date back of the second millenary before the Christian era, and immediately preceded the Roman civilization; and that, at the time of the subjugation of Noricum by Rome, the manufacture of bronze already existed.

At the close of his address, Virchow merely touched upon other anthropological questions, and pointed out that philology and archeology alone were not in condition to relieve the darkness which still concealed the invention and spread of bronze; and that somatic anthropology, i.e., the investigation of the physical constitution of the peoples under consideration, as seen from the bones preserved to us, may here have a final word to say, and may, perhaps, answer the important question, whether the cultivation of central Europe is to be traced to the influence of two different families, or to only one, the Aryan.

THE VEGETATION OF THE CARBONIFEROUS AGE.[1]

MUCH of the second decade of my life was spent in the practical pursuit of geology in the field; and throughout most of that period I enjoyed almost daily intercourse with William Smith, the father of English geology. But, in later years, circumstances restricted my studies to the paleontological side of the science: hence I was anxious that the council of the British association should place in this chair some one more familiar than myself with the later developments of geographical geology. But my friend, Professor Bonney, failing to recognize the force of my objections, intimated to me that I might render some service to the association by placing before you a sketch of the present state of our knowledge of the vegetation of the carboniferous age.

This being a subject respecting which I have formed some definite opinions, I am going to act upon the suggestion. To some this may savor of 'shop-talk;' but such is often the only talk which a man can indulge in intelligently: and to close his mouth on his special themes may compel him either to talk nonsense or to be silent.

Whilst undertaking this task, I am alive to the difficulties which surround it, especially those arising from the wide differences of opinion amongst paleobotanists on some fundamental points. On some of the most important of these there is a substantial agreement between the English and German paleontologists. The dissentients are chiefly, though not entirely, to be found amongst those of France, who have, in my humble opinion, been unduly influenced by what is in itself a noble motive; viz., a strong reverence for the views of their illustrious teacher, the late Adolphe Brongniart. Such a tendency speaks well for their hearts; though it may, in these days of rapid scientific progress, seriously mislead their heads. I shall, however, endeavor to put before you faithfully the views entertained by my distinguished French friends, M. Renault, M. Grand-Eury, and the Marquis of Saporta, giving, at the same time, what I deem to be good reasons for not agreeing with them. I believe that many of our disagreements arise from geological differences between the French carboniferous strata and those in our own islands. There are some important types of carboniferous plants that appear to be much better represented amongst us than in France: hence we have, I believe, more abundant material than the French paleontologists possess, for arriving at sound conclusions respecting these plants. We have rich sources, supplying specimens in which the internal organization is preserved, in eastern Lancashire and western Yorkshire, Arran, Burntisland, and other scattered localities: France has equally rich localities at Autun and at St. Étienne. But some important difference exists between these localities. The French objects are preserved in an impracticable siliceous matrix, extremely troublesome to work, except in specimens of small size: ours, on the other hand, are chiefly embedded in a calcareous material, which, whilst it preserves the objects in an exquisite manner, does not prevent our dissecting examples of considerable magnitude. But, besides this, we are much richer in huge Lepidodendroid and Sigillarian trees, with their Stigmarian roots, than the French are: hence we have a vast mass of material illustrating the history of these types of vegetation, in which they seem to be seriously deficient. This fact alone appears to me sufficient to account for many of the wide differences of opinion that exist between us, respecting these trees. My second difficulty springs out of the imperfect state of our knowledge of the subject. One prominent cause of this imperfection lies in the state in which our specimens are found. They are not only too frequently fragmentary, but most of those fragments only present the external forms of the objects. Now, mere external forms of fossil plants are somewhat like similarities of sound in the comparative study of languages: they are too often unsafe guides. On the other hand, microscopic internal organizations in the former subjects are like grammatical indentities in the latter one: they indicate deep affinities that promise to guide the student safely to philosophical

[1] Opening address before the section of geology of the British association for the advancement of science. By Prof. W. C. WILLIAMSON, LL.D., F.R.S., president of the section. From advance sheets kindly furnished by the editor of *Nature*.

conclusions. But the common state in which our fossil plants are preserved presents a source of error that is positive as well as negative. Most of those from our coal-measures consist of inorganic shale, sandstone, or ironstone, invested by a very thin layer of structureless coal. The surface of the inorganic substance is moulded into some special form, dependent upon structural peculiarities of the living plants; which structures were sometimes external, sometimes internal, and sometimes intermediate ones. Upon this inorganic cast we find the thin film of structureless coal, which, though of organic origin, is practically as inorganic as the clay or sandstone which it invests; but its surface displays specific sculpturings, which are apt to be regarded as always representing the outermost surface of the plant when living, whereas this is not always the case. That the coaly film is a relic of the carbonaceous substance of the living plant is unquestionable; but the thinnest of these films are often the sole remaining representatives of structures that must originally have been many inches, and in some instances even many feet, in thickness. In such cases most of the organic material has been dissipated, and what little remains has often been consolidated in such a way that it is merely moulded upon the sculptured inorganic substance which it covers, and hence affords no information respecting the exterior of the fossil when a living organism. It is, in my opinion, from specimens like these, that the smooth bark of the Calamite has been credited with a fluted surface, and the Trigonocarpons with a merely triangular exterior and a misleading name, as it long caused the inorganic casts known as Sternbergiae to be deemed a strange form of plant, that had no representative amongst living types. In other cases the outermost surface of the bark is brought into close contact with the surface of the vascular cylinder. I have a Stigmaria in which the bases of the rootlets appear to be planted directly upon that cylinder, the whole of the thick intermediate bark having disappeared. In other examples, that vascular zone has also gone. Thus the innermost and outermost surfaces of a cylinder, originally many inches apart, are, through the disappearance of the intermediate structures, brought into close approximation. In such cases, leaves and other external appendages appear to spring directly from what is merely an inorganic cast of the interior of the pith. I believe that many of our Calamites are in this condition. Such examples have suggested the erroneous idea that the characteristic longitudinal flutings belong to the exterior of the bark.

Fungi. — Entering upon a more detailed review of our knowledge of the carboniferous plants, and commencing at the bottom of the scale, we come to the lowly group of the fungi, which are unquestionably represented by the Peronosporites antiquarius[1] of Worthington Smith. There seems little reason for doubting that this is one of the phycomycetous fungi, possibly somewhat allied to the Saprolegnieae; but since we have, as yet, no evidence respecting its fructification, these closer relationships must for the present remain undetermined. So far as I know, this is the only fungus satisfactorily proved to exist in the carboniferous rocks, unless the Excipulites Neesii of Goeppert, and one or two allied forms, belong to the fungoid group. The Polyporites Bowmanni is unquestionably a scale of a holoptychian fish.

Algae. — Numerous objects supposed to belong to this family have been discovered in much older rocks than carboniferous ones. The subject is a thorny one. That marine plants of some kind must have existed simultaneously with the molluscous and other plant-eating animals of paleozoic times, is obviously indisputable; but what those plants were is another question. The widest differences of opinion exist in reference to many of them. A considerable number of those recognized by Schimper, Saporta, and other paleobotanists, are declared by Nathorst to be merely inorganic tracks of marine animals; and, in the case of many of these, I have little doubt that the Swedish geologist is right. Others have been shown to be imperfectly preserved fragments of plants of much higher organization than algae, branches of conifers even being included amongst them. I have, as yet, seen none of carboniferous age that could be indisputably identified with the family of algae, though there are many that look like and may probably be such. The microscope alone can settle this question, though even this instrument fails to secure unity of opinion in the case of Dawson's Prototaxites; and no other of the supposed seaweeds hitherto discovered have been sufficiently well preserved to bear the microscopic test: hence I think that their existence in carboniferous rocks can only be regarded as an unproven probability. Mere superficial resemblances do not satisfy the severe demands of modern science, and probabilities are an insufficient foundation upon which to build evolutionary theories.

Seeing what extremely delicate cell-structures *are* preserved in the carboniferous beds, it cannot appear other than strange that the few imperfect fungoid relics just referred to constitute the only terrestrial cellular cryptogams that have been discovered in the carboniferous strata. The Darwinian doctrine would suggest that these lower forms of plant-life ought to have abounded in that primeval age; and that they were capable of being preserved is proved by the numerous specimens met with in tertiary deposits. Why we do not find such in the paleozoic beds is still an unsolved problem.

Vascular cryptogams. — The vascular cryptogams, next to be considered, burst upon us almost suddenly, and in rich profusion, during the Devonian age. They are equally silent in the Devonian and carboniferous strata as to their ancestral descent.

Ferns. — The older taxonomic literature of paleozoic fern-life is, with few exceptions, of little scientific value. Hooker and others have uttered in vain wise protests against the system that has been pursued. Small fragments have had generic and specific names assigned to them, with supreme indifference to the study of morphological variability amongst living types. The undifferentiated tip of a terminal

[1] Memoir xi. p. 299.

pinnule has had its special name, whilst the more developed structures forming the lower part of a frond have supplied two or three more species. Then the distinct forms of the fertile fronds may have furnished additional ones, whilst a further cause of confusion is seen in the wide difference existing between a young, half-developed seedling and the same plant at an advanced stage of its growth. Any one who has watched the development of a young Polypodium aureum can appreciate this difference. Yet, in the early stages of paleontological research, observers could scarcely have acted otherwise than as they did, in assigning names to these fragments, if only for temporary working purposes. Our error lies in misunderstanding the true value of such names. At present the study of fossil ferns is affording some promise of a newer and healthier condition. We are slowly learning a little about the fructification of some species, and the internal organization of others. Facts of these kinds, cautiously interpreted, are surer guides than mere external contours. Unfortunately, such facts are, as yet, but few in number; and, when we have them, we are too often unable to identify our detached sporangia, stems, and petioles, with the fronds of the plants to which they primarily belonged.

That all the carboniferous plants included in the genera Pecopteris, Neuropteris, and Sphenopteris, are ferns, appears to be most probable; but what the true affinities of the objects included in these ill-defined genera may be, is very doubtful. Here and there we obtain glimpses of a more definite kind. That the Devonian Palaeopteris hibernica is an hymenophyllous form appears to be almost certain; and, on corresponding grounds, we may conclude that the carboniferous forms, Sphenopteris trichomanoides, S. Humboldtii,[1] and Hymenophyllum Weissii,[2] belong to the same group. The fructification of the two latter leaves little room for doubting their position, whilst the foliage of some other species of Sphenopteris is suggestive of similar conclusions; but, until their fructification is discovered, this cannot be determined. An elegant form of Sphenopteris (S. tenella, Brong.; S. lanceolata of Gutbier), recently described by Mr. Kidson of Stirling, abundantly justifies caution in dealing with these Sphenopterides. This plant possesses a true sphenopteroid foliage, but its fructification is that of a marattiaceous danaid. The sporangia are elongated vertically, and have the round terminal aperture of both the recent and fossil Danaiae, — a group of plants far removed from the hymenophyllaceous type of sphenopterid already referred to.

Whether or not this Sphenopteris was really marattiaceous in other features than in its fructification, is uncertain; but I think that we have indisputably got stems and petioles of Marattiaceae from the carboniferous strata. My friend M. Renault, and I, without being aware of the fact, simultaneously studied the Medullosa elegans of Colta. This plant was long regarded as the stem of a true monocotyledon, — a decision the accuracy of which was doubted first by Brongniart, and afterwards by Binney. M. Renault's memoir, and my part vii., appeared almost simultaneously. We then found that we had alike determined the supposed monocotyledon to be not only a fern, but to belong to the peculiarly aberrant group of the Marattiaceae. As yet we know nothing of its foliage and fructification.

[1] Schimper, vol. i. p. 408. [2] *Ibid.*, p. 415.

M. Grand-Eury has figured[1] a remarkable series of ferns from the coal-measures of the basin of the Loire, the sporangia of which exhibit marked resemblances to those of the Marattiaceae. This is especially the case with his specimens of Asterotheca and Scolecopteris,[2] as also with his Pecopteris Marattiaetheca, P. Angiotheca, and P. Danaeaetheca; but there is some doubt as to the dehiscence of the sporangia of these plants: hence their marattiaceous character is not absolutely established.

That the coal-measures contain the remains of arborescent ferns has long been known, especially from their abundance at Autun. In Lancashire I have only met with the stems or petioles of one species preserving their internal organization.[3] The Rev. H. H. Higgins obtained stems that appear to have been tree-ferns from Ravenhead, in Lancashire; and it is probable that most of the plants included in the genera Psaronius, Caulopteris, and Protopteris, are also tree-ferns.

There yet remains another remarkable group of ferns, the sporangia of which are known to us through the researches of M. Renault. In these the fertile pinnules are more or less completely transmuted into small clusters of oblong sporangia. In one case, M. Renault believes that he has identified these organs with a stem or petiole of a type not uncommon at Oldham and Halifax, belonging to Corda's genus Zygopteris. Renault has combined this with some others to constitute his group of Botryopteridées, an altogether extinct and generalized type. This review shows, that whilst forms identifiable with the Hymenophyllaceae and Marattiaceae existed in the carboniferous epoch, and we find here and there traces of affinities with some other more recent types, most of the carboniferous ferns are generalized primeval forms, which only become differentiated into later ones in the slow progress of time.

Equisetaceae and Asterophylliteae (*Brongniart*), *Calamariae* (*Endlicher*), *Equisetineae* (*Schimper*). — Confusion culminates in the history of this variously named group: hence the subject is a most difficult one to treat in a concise way. The confusion began when Brongniart separated the plants contained in the group into two divisions, one of which (Équisétacés) he identified with the living equisetums, and the other (Astérophyllitées) he regarded as being gymnospermous dicotyledons. To Schimper belongs the merit, as I believe it to be, of steadily resisting this division; nevertheless, paleobotanists are still

[1] Flore carbonifère du Départment de la Loire et du centre de la France.

[2] *Loc. cit.*, tab. viii., figs. 1–5.

[3] Psaronius Renaultii, Memoir vii., p. 10; and Memoir xii., pl. iv., fig. 16. These and other similar references are to my series of memoirs on the organization of the fossil plants of the coal-measures, published in the Philosophical transactions.

separated into two schools on the subject. Dawson, Renault, Grand-Eury, and Saporta adhere to the Brongniartian idea; whilst the British and German paleontologists have always adopted the opposite view, rejecting the idea that any of these plants were other than cryptogams.

A fundamental feature of the entire group is in the fact that their foliar appendages, however morphologically and physiologically modified, are arranged in nodal verticils. This appears to be the only characteristic which the plants possess in common.

Calamites and Calamodendron.— In his 'Prodrome' (1828), and in his later 'Végétaux fossiles,' Brongniart adopted the former of these generic names as previously employed by Suckow, Schlotheim, Sternberg, and Artis. It was only in his 'Tableau des genres de végétaux fossiles' (Dictionnaire universel d'histoire naturelle, 1849) that he divided the genus, introducing the second name to represent what he believed to be the gymnospermous division of the group. A long series of investigations, extending over many years, has convinced me that no such gymnospermous type exists.[1] The same conclusion has more recently been arrived at by Vom c. M. D. Stur,[2] after studying many continental examples in which structure is preserved. What I regard as *an* error appears to have had an intelligible origin, — the fertile source of similar errors in other groups.

Nearly all the Calamitean fossils found in shales and sandstones consist of an inorganic, superficially fluted substance, coated over with a thin film of structureless coal (see 'Histoire des végétaux fossiles,' vol. i. pl. 22); *the latter being exactly moulded upon and following the outlines of the inorganic fluted cast that underlies it.* Brongniart, and those who adopt his views, believe that the external surface of this coal-film exactly represents the corresponding external surface of the original plant: hence the conclusion was arrived at, that the plant had a very large central fistular cavity, surrounded by a very thin layer of cellular and vascular tissues, as in some living equisetums. On the other hand, Brongniart also obtained some specimens of what he primarily believed to be Calamites, in which the central pith was surrounded by a thick layer of woody tissue arranged in radiating laminated wedges, separated by medullary rays. The exogenous structure of this woody zone was too obvious to escape his practised eye. But, not supposing it possible that any cryptogam could possess a cambium-layer and an exogenous mode of development, Brongniart came to the conclusion that the thin-walled specimens found in the shales and sandstones were true Equisetaceae, those with the thick, woody cylinders being mere exogens of another type. His conclusion that they were gymnosperms was a purely hypothetical one, since justified by no one feature of their organization.

My researches, based upon a vast number of specimens of all sizes, from minute twigs little more than the thirtieth of an inch in diameter to thick stems at least thirteen inches across, led me to the conclusion that we have but one type of calamite, and that the differences which misled Brongniart are merely due to variations in the mode of their preservation.[1] It became clear to me that the outer surface of the coaly film in the specimens preserved in the shales and sandstones did *not* represent the outer surface of the living plant, but was only a fractional remnant of the carbon of that plant, which had undergone a complete metamorphosis. The greater part of what originally existed had disappeared, probably in a gaseous state; and the little that remained, displaying no organic structure, *had been moulded upon the underlying inorganic cast of the medullary cavity.* This cast is always fluted longitudinally, and constructed transversely at intervals of varying lengths. Both these features were due to impressions made by the organism upon the inorganic sand or mud filling the medullary cavity whilst it was in a plastic state, and which subsequently became more or less hardened; the longitudinal grooves being caused by the pressure of the inner angles of the numerous longitudinally vascular wedges, and the transverse ones partly by the remains of a cellular nodal diaphragm which crossed the fistular medullary cavity, and partly by a centripetal encroachment of the vascular zone at each of the same points.[2]

My cabinets contain an enormous number of sections of these plants, in which the minutest details of their organization are exquisitely preserved. These specimens, as already observed, show their structure in every stage of their growth, — from the minutest twigs, to stems more than a foot in diameter. Yet these various examples are all, without a solitary exception, constructed upon one common plan. That plan is an extremely complicated one, — far too complex to make it in the slightest degree probable that it could co-exist in two such very different orders of plants as the Equisetaceae and the Gymnospermae. Yet, though very complex, it is, even in many of its minuter details, unmistakably the plan upon which the living equisetums are constructed. The resemblances are too clear, as well as too remarkable, in my mind, to leave room for any doubt on this point. The great differences are only such as necessarily resulted from the gradual attainment of the arborescent form so unlike the lowly herbaceous one of their living representatives. On the other hand, no living gymnosperm possesses an organization that *in any solitary feature* resembles that of the so-called Calamodendra. The two have absolutely nothing in common: hence the conclusion that these Calamodendra were gymnospermous plants is as arbitrary an assumption as could possibly be forced upon science, — an assumption that no arguments derived from the merely external aspects of structureless specimens could ever induce me to accept.

These Calamites exhibit a remarkable morphological characteristic, which presents itself to us here for the first time, but which we shall find recurs in other paleozoic forms. Some of our French botani-

[1] Memoirs i., ix., and xii.

[2] Zur morphologie der calamarien.

[1] Memoirs i. and ix.

[2] See Memoir i., pl. xxiv., fig. 10; and pl. xxvi., fig. 24.

cal friends group the various structures contained in plants into several 'appareils,'[1] distinguished by the functions which those structures have to perform. Amongst others, we find the 'appareil de soutiens,' embracing those hard, woody tissues which may be regarded as the supporting skeleton of the plant, and the 'appareil conducteur,' which M. van Tieghem describes as composed of two tissues, — "le tissu criblé qui transporte essentiellement les matières insolubles, et le tissu vasculaire qui conduit l'eau et les substances dissoutes." Without discussing the scientific limits of this definition, it suffices for my present purpose. In nearly all flowering plants these two 'appareils' are more or less blended. The supporting wood-cells are intermingled in varying degrees with the sap-conducting vessels. It is so, even in the lower gymnosperms; and in the higher ones these wood-cells almost entirely replace the vessels. It is altogether otherwise with the fossil cryptogams. The vascular cylinder in the interior of the Calamites, for example, consists wholly of *barred* vessels, a slight modification of the scalariform type so common in all cryptogams. No trace of the 'appareil de soutiens' is to be found amongst them. The vessels are, in the most definite sense, the 'appareils conducteurs' of these plants. No such absolutely undifferentiated unity of tissue is to be found in any living plants other than cryptogams.

But these Calamites, when living, towered high into the air. My friend and colleague, Professor Boyd Dawkins, recently assisted me in measuring one found in the roof of the Moorside colliery, near Ashton-under-Lyne, by Mr. George Wild, the very intelligent manager of that and some neighboring collieries. The flattened specimen ran obliquely along the roof, each of its two extremities passing out of sight, burying themselves in the opposite sides of the mine. Yet the portion which we measured was thirty feet long; its diameter being six inches at one end, and four inches and a half at the other. The mean length of its internodes at its broader end was three inches, and at its narrower one an inch and a half. What the real thickness of this specimen was when all its tissues were present, we have no means of judging; but the true diameter of the cylinder represented by the fossil when uncompressed has been only four inches at one end of the thirty feet, and two inches and a half at the other. Whatever its entire diameter when living, the vascular cylinder of this stem must have been at once tall and slender, and consequently must have required some 'appareil de soutien' such as its exogenous vascular zone did not supply. This was provided in a very early stage of growth by the introduction of a second cambium-layer into the bark; which, though reminding us of the cork-cambium in ordinary exogenous stems, produced, not cork, but prosenchymatous cells.[2] In its youngest state, the bark of the Calamites was a very loose cellular parenchyma; but in the older stems much of this parenchyma became enclosed in the prosenchymatous tissue referred to, and which appears to have constituted the greater portion of the matured bark. The sustaining skeleton of the plant, therefore, was a hollow cylinder, developed centrifugally on the inner side of an enclosing cambium-zone. That this cambium-zone must have had some protective periderm external to *it*, is obvious; but I have not yet discovered what it was like. We shall find a similar cortical provision for supporting lofty cryptogamous stems in the Lepidodendra and Sigillariae.

[1] Van Tieghem, Traité de botanique, p. 679.
[2] Memoir ix., pl. xx., figs. 14, 15, 18, 19, and 20.

The carboniferous rocks have furnished a large number of plants having their foliage arranged in verticils, and which have had a variety of generic names assigned to them. Such are Asterophyllites, Sphenophyllum, Annularia, Bechera, Hippurites, and Schizoneura. Of these genera, Sphenophyllum is distinguished by the small number of its wedge-shaped leaves; and the structure of its stems has been described by M. Renault. Annularia is a peculiar form, in which the leaves forming each verticil, instead of being all planted at the same angle upon the central stem, are flattened obliquely nearly in the plane of the stem itself. Asterophyllites differs from Sphenophyllum chiefly in the larger number and in the linear form of its leaves. Some stems of this type have virtually the same structure[1] as those of Sphenophyllum, — a structure which differs widely from that of the Calamites, and of which, consequently, these plants cannot constitute the leaf-bearing branches. But there is little doubt that true calamitean branches have been included in the genus Asterophyllites. I have specimens, for which I am indebted to Dr. Dawson, which I should unhesitatingly have designated Asterophyllites but for my friend's positive statement that he detached them from stems of a calamite. Of the internal organization of the stems of the other genera named, we know nothing.

It is a remarkable fact, that notwithstanding the number of young calamitean shoots that we have obtained from Oldham and Halifax, in which the structure is preserved, we have not met with one with the leaves attached. This is apparently due to the fact that most of the specimens are decorticated ones. We have a sufficient number of corticated specimens to show us what the bark was, but such specimens are not common. They clearly prove, however, that their bark had a smooth, and not a furrowed, external surface.

There yet remains for consideration the numerous reproductive strobili, generally regarded as belonging to plants of this class, Equisetinae. We find some of these strobili associated with stems and foliage of known types, as in Sphenophyllum;[2] but we know nothing of the internal organization of these sphenophylloid strobili. We have strobili connected with stems and foliage of Annularia,[3] but we are equally ignorant of the organization of these. So far as that

[1] Memoir, part v., pl. i.–v.; and part ix., pl. xxi., fig. 32.
[2] Lesquereux, Coal flora of Pennsylvania, pl. ii., fig. 687.
[3] Ueber die fruchtähren von Annularia sphenophylloides. Von T. Sterzel. Zeitschr. d. deutschen geolog. gesellschaft., Jahrg. 1882.

organization can be ascertained from Sterzel's specimen, it seems to have alternating sterile and fertile bracts, with the sporangia of the latter arranged in fours, as in Calamostachys.[1] On the other hand, we are now very familiar with the structure of the Calamostachys Binneana, the prevalent strobilus in the calcareous nodules found in the lower coal-measures of Lancashire and Yorkshire. It has evidently been a sessile spike, the axial structures of which were trimerous[2] (rarely tetramerous), having a cellular medulla in its centre. Its appendages were exact multiples of those numbers. Of the plant to which it belonged we know nothing. On the other hand, we have examples supposed to be of the same genus, as C. paniculata[3] and C. polystachya,[4] united to stems with asterophyllitean leaves; but whether or not these fruits have the organization of C. Binneana, we are unable to say.

We are also acquainted with the structure of the two fruits belonging to the genera Bruckmannia[5] and Volkmannia.[6] This latter term has long been very vaguely applied.

There still remain the genera Stachannularia, Palaeostachya, Macrostachya, Cingularia, Huttonia, and Calamitina, all of which have the phyllomes of their strobili fertile and sterile, arranged in verticils, and some of them display asterophyllitean foliage. But these plants are only known from structureless impressions. That all these curious spore-bearing organisms have close affinities with the large group of the equisetums cannot be regarded as certain; but several of them undoubtedly have peculiarities of structure suggestive of relations with the Calamites. This is especially observable in the longitudinal canals found in the central axis of each type, apparently identical with what I have designated the internodal canals of the Calamites.[7] The position and structure of their vascular bundles suggest the same relationship, whilst in many the position of the sporangia and sporangiophores is eminently equisetiform. Renault's Bruckmannia Grand-Euryi and B. Decaisnei, and a strobilus which I described in 1870,[8] exhibit these calamitean affinities very distinctly.

One strobilus which I described in 1880[9] must not be overlooked. As is well known, all the living forms of esquisetaceous plants are isosporous. We only discover heterosporous vascular cryptogams amongst the Lycopodiaceae and the Rhizocarpae. My strobilus is identical, in every detailed feature of its organization, with the common Calamostachys Binneana, excepting that it is heterosporous; having microspores in its upper, and macrospores in its lower part, — a state of things suggestive of some link between the Equisetinae and the heterosporous Lycopodiaceae.

Lycopodiaceae. — This branch of my subject suggests memories of a long conflict, which, though it is virtually over, still leaves here and there the ground-swell of a stormy past. At the meeting of the British association at Liverpool, in 1870, I first announced that a thick, secondary, exogenous growth of vascular tissue existed in the stems of many carboniferous cryptogamic plants, especially in the calamitean and lepidodendroid forms. But at that time the ideas of M. Brongniart were so entirely in the ascendant, that my notions were rejected by every botanist present. Though the illustrious French paleontologist knew that such growths existed in Sigillariae and in what he designated Calamodendra, he concluded, that, *de facto*, such plants could not be cryptogams. Time, however, works wonders. Evidence has gradually accumulated, proving, that, with the conspicuous exception of the ferns, nearly every carboniferous cryptogam was capable of developing such zones of secondary growth. The exceptional position of the ferns still appears to be as true as it was when I first proclaimed their exceptional character at Liverpool. At that time I was under the impression that the secondary wood was only developed in such plants as attained to arboreal dimensions; but I soon afterwards discovered that it occurred equally in many small plants like Sphenophyllum, Asterophyllites, and other diminutive types.

After thirteen years of persevering demonstration, these views, at first so strongly opposed, have found almost universal acceptance; nevertheless, there still remain some few who believe them to be erroneous ones. In the later stages of this discussion the botanical relations subsisting between Lepidodendron, Sigillaria, and Stigmaria, have been the chief themes of debate. In this country we regard the conclusion, that Stigmaria is not only a root, but the root alike of Lepidodendron and Sigillaria, as settled beyond all dispute. Nevertheless, M. Renault and M. Grand-Eury believe that it is frequently a leaf-bearing rhizome, from which aerial stems are sent upwards. I am satisfied that there is not a shadow of foundation for such a belief. The same authors, along with their distinguished countryman the Marquis de Saporta, believe with Brongniart that it is possible to separate Sigillaria widely from Lepidodendron. They leave the latter plant amongst the lycopods, and elevate the former to the rank of a gymnospermous exogen. I have in vain demonstrated the existence of a large series of specimens of the same species of plant, young states of which display all the essential features of structure which they believe to characterize Lepidodendron; whilst, in its progress to maturity, every stage in the development of the secondary wood, regarded by them as characteristic of a Sigillaria, can be followed step by step.[1] Nay, more. My cabinet contains specimens of young dichotomously branching twigs, on

[1] M. Renault has described a strobilus under the name of Annularia longifolia, but which appears to me very distinct from that genus.

[2] It is an interesting fact, that transverse sections of the strobili of Lycopodium alpinum exhibit a similar trimerous arrangement, though differing widely in the positions of its sporangia.

[3] Weiss, Abhandlungen zur geologischen specialkarte von Preussen und Thürinaischen Staaten, taf. xiii., fig 1.

[4] *Idem*, taf. xvi., figs. 1, 2.

[5] Renault, Annales de sciences naturelles, bot., tome iii., pl. iii.

[6] *Idem*, pl. ii.

[7] Memoir i.

[8] Memoirs of the literary and philosophical society of Manchester, 3d series, vol. iv. p. 245.

[9] Memoir xi., pl. liv., figs. 23, 24.

[1] Memoir xi., plates xlvii. - lii.

which one of the two diverging branches has only the centripetal cylinder of the Lepidodendron, whilst the other has begun to develop the secondary wood of the Sigillaria.[1]

The distinguished botanist of the Institut, Ph. van Tieghem, has recently paid some attention to the conclusions adopted by his three countrymen in this controversy, and has made an important advance upon those conclusions, in what I believe to be the right direction. He recognizes the lycopodiaceous character of the Sigillariae, and their close relations to the Lepidodendra;[2] and he also accepts my demonstration of the unipolar, and consequently lycopodiaceous, character of the fibro-vascular bundle of the stigmarian rootlet, — a peculiarity of structure of which M. Renault has hitherto denied the existence. But along with these recognitions of the accuracy of my conclusions, he gives fresh currency to several of the old errors relating to parts of the subject to which he has not yet given personal attention. Thus he considers that the Sigillariae, though closely allied to the Lepidodendra, are distinguished from them by possessing the power of developing the centrifugal or exogenous zone of vascular tissue already referred to. He characterizes the Lepidodendra as having '*un seul bois centripète*,' notwithstanding the absolute demonstrations to the contrary contained in my Memoir xi. Dealing with the root of Sigillaria, which in Great Britain, at least, is the well-known Stigmaria ficoides, following Renault, he designates it a 'rhizome,' limiting the term 'root' to what we designate the rootlets. He says, "Le rhizome des sigillaires a la même structure que la tige aérienne, avec des bois primaires tantôt isolés à la périphérie de la moelle, tantôt confluents au centre et en un ax plein; seulement les fasceaux libéro-ligneux secondaires y sont séparés par de plus larges rayons," etc.

Now, Stigmaria, being a root, and not a rhizome, contains no representative of the primary wood of the stem. This latter is, as even M. Brongniart so correctly pointed out long ago, the representative of the medullary sheath; and the fibro-vascular bundles which it gives off are all foliar ones, as is the case with the bundles given off by this sheath in all exogenous plants. But in the Lepidodendra and Sigillariae, as in all living exogens, it is not prolonged into the root. In the latter, as might be expected *a priori*, we only find the secondary or exogenous vascular zone. Having probably the largest collection of sections of Stigmariae in the world, I speak unhesitatingly on these points. M. van Tieghem further says, "La tige aérienne part d'un rhizome rameux très-développé nommé Stigmaria, sur lequel s'insèrent à la fois de petites feuilles et des racines parfois dichotomées." I have yet to see a solitary fact justifying the statement that leaves are intermingled with the rootlets of Stigmaria. The statement rests upon an entire misinterpretation of sections of the fibro-vascular bundles supplying those rootlets, and an ignorance of the nature and positions of the rootlets themselves. More than forty years have elapsed since John Eddowes Bowman first demonstrated that the Stigmariae were true roots; and every subsequent British student has confirmed Bowman's accurate determination.

[1] Memoir xi., pl. xlix., fig. 8.

[2] Traité de botanique, p. 304.

M. Lesquereux informs me that his American experiences have convinced him that Sigillaria is lycopodiaceous. Dr. Dawson has now progressed so far in the same direction as to believe that there exists a series of sigillarian forms which link the Lepidodendra on the one hand with the gymnospermous exogens on the other. As an evolutionist, I am prepared to accept the possibility that such links may exist. They certainly do, so far as the union of Lepidodendron with Sigillaria is concerned. I have not yet seen any from the higher part of the chain that are absolutely satisfactory to me, but Dr. Dawson thinks that he has found such. I may add, that Schimper and the younger German school have always associated Sigillaria with the Lycopodiaceae; but there are yet other points under discussion connected with these fossil lycopods.

M. Renault affirms that some forms of Halonia are subterranean rhizomes, and the late Mr. Binney believed that Haloniae were the roots of Lepidodendron. I am not acquainted with a solitary fact justifying either of these suppositions, and unhesitatingly reject them. We have the clearest evidence that some Haloniae, at least, are true terminal, and, as I believe, strobilus-bearing, branches of various lepidodendroid plants; and I see no reason whatever for separating Halonia regularis from those whose fruit-bearing character is absolutely determined. Its branches, like the others, are covered throughout their entire circumference, and in the most regularly symmetrical manner, with leaf-scars, — a feature wholly incompatible with the idea of the plant being either a root or a rhizome. M. Renault has been partly led astray in this matter by misinterpreting a figure of a specimen published by the late Mr. Binney. That specimen being now in the museum of Owens college, we are able to demonstrate that it has none of the features which M. Renault assigns to it.

The large, round or oval, distichously arranged scars of Ulodendron have long stimulated discussion as to their nature. This, too, is now a well-understood matter. Lindley and Hutton long ago suggested that they were scars whence cones had been detached, — a conclusion which was subsequently sustained by Dr. Dawson and Schimper,[1] and which structural evidence led me also to support. The matter was set at rest by Mr. d'Arcy Thompson's discovery of specimens with the strobili *in situ*. Only a small central part of the conspicuous cicatrix characterizing the genus represented the area of organic union of the cone to the stem. The greater part of that cicatrix has been covered with foliage, which, owing to the shortness of the cone-bearing branch, was compressed by the base of the cone. The large size of many of these biserial cicatrices on old stems has been due to the considerable growth of the stem subsequently to the fall of the cone.

Our knowledge of the terminal branches of the

[1] Memoir ii., p. 222.

large-ribbed Sigillariae is still very imperfect. Paleontologists who have urged the separation of the Sigillariae from the Lepidodendra have attached weight to the difference between the longitudinally ridged and furrowed external bark of the former plants, along which ridges the leaf-scars are disposed in vertical lines, and the diagonally arranged scars of Lepidodendron. They have also dwelt upon the alleged absence of branches from the sigillarian stems. I think that their mistake, so far as the branching is concerned, has arisen from their expectation that the branches must necessarily have had the same vertically grooved appearance and longitudinal arrangement of the leaf-scars as they observed in the more aged trunks: hence they have probably *seen* the branches of Sigillariae without *recognizing* them. Personally, I believe this to have been the case. I further entertain the belief, that the transition from the *vertical* phyllotaxis, or leaf-arrangement, of the sigillarian leaf-scars, to the *diagonal* one of the Lepidodendra, will ultimately be found to be effected through the subgenus Favularia, in many of which the diagonal arrangement becomes quite as conspicuous as the vertical one. This is the case even in Brongniart's classic specimen of Sigillaria elegans, long the only fragment of that genus known, which preserved its internal structure. The fact is, the shape of the leaf-scars, as well as their proximity to each other, underwent great changes as lepidodendroid and sigillarian stems advanced from youth to age. Thus Presl's genus Bergeria was based on forms of lepidodendroid scars which we now find on the terminal branches of unmistakable lepidodendra.[1] The phyllotaxis of Sigillaria, of the type of S. occulata, passes by imperceptible gradations into that of Favularia. In many young branches the leaves were densely crowded together; but the exogenous development of the interior of the stem, and its consequent growth both in length and thickness, pushed these scars apart at the same time that it increased their size and altered their shape. We see precisely the same effects produced upon the large fruit-scars of Ulodendron by the same causes. The carboniferous lycopods were mostly arborescent; but some few dwarf forms, apparently like the modern Selaginellae, have been found in the Saarbrücken coal-fields. Many, if not all, the arborescent forms produced secondary wood by means of a cambium-layer, as they increased in age. In the case of some of them,[2] this was done in a very rudimentary manner; nevertheless, sufficiently so to demonstrate what is essential to the matter, viz., the existence of a cambium-layer producing 'centrifugal growth of secondary vascular tissue.'

As already pointed out in the case of the Calamites, the vascular axis of these Lepidodendra was purely an 'appareil conducteur,' unmixed with any wood-cells: hence the 'appareil de soutien' had to be supplied elsewhere. This was done as in the Calamites: a thick, persistent, hypodermal zone of meristem[3] developed a layer of prismatic prosenchyma of enormous thickness,[1] which incased the softer structures in a strong cylinder of self-supporting tissue. We have positive evidence that the fructification of many of these plants was in the form of heterosporous strobili. Whether or not such was the case with all the Lepidostrobi, we are yet unable to determine; but the incalculable myriads of their macrospores, seen in so many coals, afford clear evidence that the heterosporous types must have preponderated vastly over all others.

Gymnosperms. — Our knowledge of this part of the carboniferous vegetation has made great progress during the last thirty years. This progress began with my own discovery[2] that all our British Dadoxylons possessed what is termed a discoid pith, such as we see in the white jasmine, some of the American hickories, and several other plants. At the same time, I demonstrated that most of our objects hitherto known as Artisias and Sternbergias were merely inorganic casts of these discoid medullary cavities. Further knowledge of this genus seems to suggest that it was not only the oldest of the true conifers in point of time, but also one of the lowest of the coniferous types.

Cycads. — The combined labors of Grand-Eury, Brongniart, and Renault, have revealed the unexpected predominance in some localities of a primitive but varied type of cycadean vegetation. Observers have long been familiar with certain seeds known as Trigonocarpons and Cardiocarpons, and with large leaves to which the name of Noeggerathia was given by Sternberg. All these seeds and leaves have been tossed from family to family at the caprice of different classifiers, but, in all cases, without much knowledge on which to base their determinations. The rich mass of material disinterred by M. Grand-Eury at St. Étienne, and studied by Brongniart and M. Renault, has thrown a flood of light upon some of these objects, which now prove to be primeval types of cycadean vegetation.

Mr. Peach's discovery of a specimen demonstrating that the Antholithes Pitcairniae[3] of Lindley and Hutton was not only, as these authors anticipated, 'the inflorescence of some plant,' but that its seeds were the well-known Cardiocarpons, was the first link in an important chain of new evidence. Then followed the rich discoveries at St. Étienne, where a profusion of seeds, displaying wonderfully their internal organization, was brought to light by the energy of M. Grand-Eury; which seeds M. Brongniart soon pronounced to be cycadean. At the same time I was obtaining many similar seeds from Oldham and Burntisland, in which, also, the minute organiza-

[1] See Memoir xii., pl. xxxiv.
[2] E. g. L. Harcourtii, Memoir ix., pl. xlix., fig. 11.
[3] Memoir ix., pl. xxv., figs. 93, 94, 98, 99, 100, and 101.

[1] Memoir xi., pl. xlviii., fig. 4 *f f'*; Memoir ii., pl. xxix., fig. 42 *k*; Memoir iii., pl. xliii., fig. 17.
[2] On the structure and affinities of the plants hitherto known as Sternbergias. Memoirs of the literary and philosophical society of Manchester, 1851. M. Renault, in his Structure comparée de quelques tiges de la flore carbonifère, p. 285, has erroneously attributed this discovery to Mr. Dawes, including my illustration from the jasmine and juglans. Mr. Dawes' explanation was a very different one.
[3] Fossil flora, p. 82.

tion was preserved. Dawson, Newberry, and Lesquereux have also shown that many species of similar seeds, though with no traces of internal structure, occur in the coal-measures of North America.

Equally important was the further discovery by M. Grand-Eury, that the Antholithes, with their cardiocarpoid seeds, were but one form of the monoclinous catkin-like inflorescences of the Noeggerathiae, now better known by Unger's name of Cordaites. These investigations suggest some important conclusions. 1°. The vast number and variety of these cycadean seeds, as well as the enormous size of some of them, are remarkable, showing the existence of an abundant and important carboniferous vegetation, of most of which no trace has yet been discovered other than these isolated seeds. 2°. Most of the seeds exhibit the morphological peculiarity of having a large cavity (the 'cavité pollinique' of Brongniart) between the upper end of the nucelle and its investing episperm, and immediately below the micropile of the seed. That this cavity was destined to have the pollen-grains drawn into it, and be thus brought into direct connection with the apex of the nucelle, is shown by the various examples in which such grains are still found in that cavity.[1] 3°. M. Grand-Eury has shown that some of his forms of Cordaites possessed the discoid or Sternbergian pith which I had previously found in Dadoxylon. And, lastly, these Cordaites prove that a diclinous form of vegetation existed at this early period in the history of the flowering plants, but whether in a monoecious or a dioecious form we have as yet no means of determining. Their reproductive structures differ widely from the true cones borne by most cycads at the present day.

Conifers. — It has long been remarked that few real cones of conifers have hitherto been found in the carboniferous rocks, and I doubt if any such have yet been met with. Large quantities of the woody stems now known as Dadoxylons have been found, both in Europe and America. These stems present a true coniferous structure, both in the pith, medullary, sheath-wood, and bark.[2] The wood presents one very peculiar feature: its foliar bundles, though in most other respects exactly like those of ordinary conifers, are given off, not singly, but in pairs.[3] I have only found this arrangement of double foliar bundles in the Chinese gingko (Salisburia adiantifolia).[4] This fact is not unimportant when connected with another one. Sir Joseph Hooker long ago expressed his opinion that the well-known Trigonocarpons[5] of the coal-measures were the seeds of a conifer allied to this Salisburia. The abundance of the fragments of Dadoxylon, combined with the readiness with which cones and seeds are preserved in a fossil state, makes it probable that the fruits belonging to these woody stems would be so preserved; but of cones we find no trace, and, as we discover no other plant in the carboniferous strata to which the Trigonocarpons could with any probability have belonged, these facts afford grounds for associating them with the Dadoxylons. These combined reasons — viz., the structure of the stems with their characteristic foliar bundles, and the gingko-like character of the seeds — suggest the probability that these Dadoxylons, the earliest of known conifers, belonged to the Taxineae, the lowest of these coniferous types, and of which the living Salisburia may perhaps be regarded as the least advanced form.

Thus far our attention has been directed only to plants whose affinities have been ascertained with such a degree of probability as to make them available witnesses, so far as they go, when the question of vegetable evolution is *sub judice*. But there remain others, and probably equally important ones, respecting which we have yet much to learn. In most cases we have only met with detached portions of these plants, such as stems or reproductive structures, which we are unable to connect with their other organs. The minute tissues of these plants are preserved in an exquisite degree of perfection: hence we are able to affirm, that, whatever they may be, they differ widely from every type that we are acquainted with amongst living ones. The exogenous stems or branches from Oldham and Halifax which I described under the name of Astromyelon,[1] and of which a much fuller description will be found in my forthcoming Memoir xii., belong to a plant of this description. The remarkable conformation of its bark obviously indicates a plant of more or less aquatic habits, since it closely resembles those of Myriophyllum, Marsilea, and a number of other aquatic plants belonging to various classes. But its general features suggest nearer affinities to the latter genus than to any other. Another very characteristic stem is the Heterangium Grievii,[2] only found in any quantity at Burntisland, but of which we have recently obtained one or two small specimens at Halifax. This plant displays an abundant supply of primary, isolated, vascular bundles, surrounded by a very feeble development of secondary vascular tissue. Still more remarkable is the Lyginodendron Oldhamium,[3] a stem not uncommon at Oldham, and not unfrequently found at Halifax. Unlike the Heterangium, its primary vascular elements are feeble, but its tendency to develop secondary zylem is very characteristic of the plant. An equally peculiar feature is seen in the outermost layer of its cellular bark, which is penetrated by innumerable longitudinal laminae of prosenchymatous tissue, which is arranged in precisely the same way as is the hard bast in the lime and similar trees, affording another example of the introduction into the outer bark of the 'appareil de soutien.' As might have been anticipated from this addition to the bark, this plant attained arborescent dimensions, very large

[1] Memoir viii., pl. ii., figs. 70 and 72. Brongniart, Recherches sur les graines fossiles silicifiées, pl. xvi., figs. 1, 2; pl. xx., fig. 2.

[2] Dr. Dawson finds the discoid pith in one of the living Canadian conifers.

[3] Memoir viii., pl. lviii., fig. 48; and pl. ix., figs. 44–46.

[4] Memoir xii., pl. xxxiii., figs. 28, 29.

[5] Memoir viii., figs. 94–115.

[1] Memoir ix., in which I only described decorticated specimens. Messrs. Cash and Heik described a specimen in which the peculiar bark was preserved under the name of Astromyelon Williamsonis. See Proceedings of the Yorkshire polytechnic society, vol. vii. part iv., 1881.

[2] Memoir iii. [3] *Ibid.*

fragments of sandstone casts of the exterior surface of the bark[1] being very abundant in most of the leading English coal-fields. Corda also figured it[2] from Radnitz, confounding it, however, with his lepidodendroid Sagenaria fusiformis, with which it has no true affinity. Of the smaller plants of which we know the structure, but not the systematic position, I may mention the beautiful little Kaloxylons.[3] We have also obtained a remarkable series of small spherical bodies, to which I have given the provisional generic name of Sporocarpon.[4] Their external wall is multicellular: hence they cannot be spores. Becoming filled with free cells, which display various stages of development as they advance to maturity, we may infer that they are reproductive structures. Dr. Dawson informs me that he has recently obtained some similar bodies, also containing cells, from the Devonian beds of North and South America. Except in calling attention to some slight resemblance existing between my objects and the sporangiocarps of Pilularia,[5] I have formed no opinion respecting their nature. Dr. Dawson has pointed out that his specimens, also, are suggestive of relations with the Rhizocarpae.

I am unwilling to close this address without making a brief reference to the bearing of our subject upon the question of evolution. Various attempts have been made to construct a genealogical tree of the vegetable kingdom. That the cryptogams and the gymnosperms made their appearance, and continued to flourish on this earth, long prior to the appearance of the monocotyledonous and dicotyledonous flowering plants, is, at all events, a conclusion justified by our present knowledge, so far as it goes. Every one of the supposed palms, aroids, and other monocotyledons, has now been ejected from the lists of carboniferous plants, and the Devonian rocks are equally devoid of them. The generic relations of the carboniferous vegetation to the higher flowering plants found in the newer strata have no light thrown upon them by these paleozoic forms. These latter do afford us a few plausible hints respecting some of their cryptogamic and gymnospermous descendants, and we know that the immediate ancestors of many of them flourished during the Devonian age; but here our knowledge practically ceases. Of their still older genealogies, scarcely any records remain. When the registries disappeared, not only had the grandest forms of cryptogamic life that ever lived attained their highest development, but even the yet more lordly gymnosperms had become a widely diffused and flourishing race. If there is any truth in the doctrine of evolution, and especially if long periods of time were necessary for a world-wide development of lower into higher races, a terrestrial vegetation must have existed during a vast succession of epochs, ere the noble lycopods began their prolonged career. Long prior to the carboniferous age they had not only made this beginning, but during that age they had diffused themselves over the entire earth. We find them equally in the old world and in the new. We discover them from amid the ice-clad rocks of Bear Island and Spitzbergen to Brazil and New South Wales. Unless we are prepared to concede that they were simultaneously developed at these remote centres, we must recognize the incalculable amount of time requisite to spread them thus from their birthplace, wherever that may have been, to the ends of the earth. Whatever may have been the case with the southern hemisphere, we have also clear evidence that in the northern one much of this wide distribution must have been accomplished prior to the Devonian age. What has become of this pre-Devonian flora? Some contend that the lower cellular forms of plant-life were not preserved, because their delicate tissues were incapable of preservation. But why should this be the case? Such plants are abundantly preserved in tertiary strata: why not equally in paleozoic ones? The explanation must surely be sought, not in their incapability of being preserved, but in the operation of other causes. But the carboniferous rocks throw another impediment in the way of constructors of these genealogical trees. Whilst carboniferous plants are found at hundreds of separate localities, widely distributed over the globe, the number of spots at which these plants are found displaying any internal structure is extremely few. It would be difficult to enumerate a score of such spots; yet each of those favored localities has revealed to us forms of plant-life of which the ordinary plant-bearing shales and sandstones of the same localities show no traces. It seems, therefore, that, whilst there was a general resemblance in the more conspicuous forms of carboniferous vegetation from the arctic circle to the extremities of the southern hemisphere, each locality had special forms that flourished in it either exclusively, or at least abundantly, whilst rare elsewhere. It would be easy, did time allow, to give many proofs of the truth of this statement. Our experiences at Oldham and Halifax, at Arran and Burntisland, at St. Étienne and Autun, tell us that such is the case. If these few spots which admit of being searched by the aid of the microscope have recently revealed so many hitherto unknown treasures, is it not fair to conclude that corresponding novelties would have been furnished by all the other plant-producing localities, if these plants had been preserved in a state capable of being similarly investigated? I have no doubt about this matter: hence I conclude that there is a vast variety of carboniferous plants of which we have as yet seen no traces, but every one of which must have played some part, however humble, in the development of the plant races of later ages. We can only hope that time will bring these now hidden witnesses into the hands of future paleontologists. Meanwhile, though far from wishing to check the construction of any legitimate hypothesis calculated to aid scientific inquiry, I would remind every too ambitious student that there is a haste that retards rather than promotes progress, that arouses opposition rather than produces conviction, and that injures the cause of science by discrediting its advocates.

[1] Memoir iv., pl. xxvii.
[2] Flora der vorvelt, tab. 6, fig. 4.
[3] Memoir vii.
[4] Memoirs ix., x.
[5] Memoir ix., p. 348.